

Murray, Elisabeth 2002

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This is an interview with Dr. Elisabeth Murray, Chief of the XXX Section of the Laboratory of Neuropsychology of the NIMH Intramural Program held on April 9, 2002, in Bethesda, Maryland.

The interviewer is Dr. Ingrid Farreras of the NIH History Office.

Farreras: Well, as I already mentioned to you I am interested not just in the Laboratory of Neuropsychology but also in you as a scientist at NIH so why don't we start with some basic background, family, where you were born, that sort of thing...?

Murray: Where I was born? That's really going back! Well, I was born in 1952 in Syracuse, New York. I grew up in Syracuse, New York, and when I went to college I was a biology major at Bucknell University in Lewisburg, Pennsylvania. And even at that time I was interested in psychology, but I don't know why I wasn't a psychology major. I think there was no one in the department that I really clicked with or there were some special requirements for psychology majors because they were trying to deter people because so many people were choosing psychology majors.

Farreras: The Bucknell program had strong animal behavior people.

Murray: Well, ironically, they did, and there were two people who were there at the time. I believe one has just retired. The main animal behavior person was Doug Candlin, who was really good, and who was probably instrumental in having me get to where I am today, but I didn't know him until later. And the other person who was certainly part of my formative years at Bucknell was Alan Leshner. Al was Deputy Director for NIMH just a few years ago, then moved on to the head of NIDA, and now has moved on to some higher position yet with AAAS. But he was a physiological psychologist and he was wonderful. I remember when he would draw graphs on the blackboards, he would be very careful about the axes. He would just have an arrow time and arrow percent or something, and I thought it was great because these are the general principles of what you need to understand, and it was true. And at the time there was no neuroscience, while now, in many undergraduate universities, there are cognitive science departments, etc. But at the time, I didn't really know the beginnings of neuroscience. But I loved all that.

Farreras: How did you end up applying to Bucknell? Where did you go to high school?

Murray: I went to Anthony A. Henniger High School in Syracuse. I think at the time it was one of the four public high schools in Syracuse. That was the one nearest me. So nothing special. I think there was no question for me. I was always interested in biology, I was always interested in animals. As a kid, I was always out looking at birds, catching frogs and bringing them home. And luckily, my grandparents had a cottage by Casanovia Lake near Syracuse, and in the summer we would sometimes spend time out at this lake, and there was a little creek nearby. So literally every morning I'm out there catching frogs, finding tadpoles. Occasionally there would be like a little opossum or snakes or something like that around.

Farreras: How fun. What about your parents? What did they do? Did your interest come from anything they did?

Murray: I don't know where it came from. Mom did the usual thing for women at that time, stayed home and raised us kids. I'm the last of four children, so about the time I was going to elementary school, she went to work at a university museum at Syracuse. And dad was a sales manager for an electrical component manufacturer in Syracuse at the time, _____ Seymour.

Farreras: So it didn't come from modeling parental occupations or interests...

Murray: No. I don't know where it came from. And even at Bucknell, I was always interested in animal behavior. I just felt more comfortable at the time being a biology major.

Farreras: But you did take psychology courses?

Murray: Oh, yes, absolutely. I didn't minor in psychology, but I certainly took a lot of psychology courses.

Farreras: Who would you say was instrumental in...

Murray: Well, Alan Leshner was just good, so he was really inspiring and interesting and just a wonderful teacher. I think it was Doug Candlin who in the end wrote me a letter of recommendation for a summer job in Texas at the Marine Biomedical Institute, that had these little summer jobs in Galveston, Texas. And who knows what the Marine Biomedical Institute was, but it sure sounded wonderful. So I applied for a summer job, Doug Candlin wrote me a letter of recommendation, and I went there. And it turned out that this is in large part a bunch of people associated with the University of Texas at Galveston, and a lot of it is neuroscience. So when I went there, I worked in the laboratory of Harry Pinsker, who's now departed, but he worked with _____ aplesia. The short story is there are a lot of people doing neuroscience, so when I got there, there were people studying all these various things in aplesia. The aplesia has a pretty small nervous system and a lot of the identified cells are like R1. You can find R1 in any garden-variety aplesia. They were studying _____-laying cells and siphon _____ reflexes, things like that. But there were other people working on the spinal thalamic tract and pain on the corticospinal system and anatomy and physiology, and when I got there there was a neurochemistry lab. So when I ran into all these different things, I just said, "This is it. This is what I want to do. This is neuroscience." But at the time there wasn't a department of neuroscience there, so I stayed there after that summer job.

Farreras: Oh, so this must have been the summer after your senior year, after you had graduated?

Murray: That's right, yes. It was my senior year at Bucknell. I got the summer job in Texas. I stayed there and worked a short year in the neurochemistry lab, and then I applied to the Physiological Department of the University of Texas medical branch in Galveston for graduate school and I just stayed there. It was all part of the University of Texas-Galveston or University of Texas Medical Branch at Galveston. It's really a medical school with a small graduate school affiliated with it, and the Marine Biomedical Institute was some other item, which I don't really understand, but I believe it's something in the University of Texas that the state of Texas funds. Many of the people at the University of Texas at Galveston have these joint appointments in the Marine Biomedical Institute, and some of them do marine-related things and some of them don't.

Farreras: Was there anyone in particular whom you wanted to work with when you applied?

Murray: No. As part of course work, you would work in a few different laboratories. They weren't official internships but I had some experience in a few different laboratories. One fellow who's moved on since that time, Joe Coulter, recruited me to his laboratory, and that worked out well. _____ there. And my dissertation did not have much to do with behavior, it was on the organization of corticospinal neurons in monkeys, so it was really on sensory _____ systems in monkeys. And I loved this neuroscience job but there was no one there really doing behavior of the kind I was interested in. There were some doing clinical neuropsychology, which I found interesting, but it wasn't quite what I had in mind. I guess I was targeting basic research, but I was just following my intuition, really. So for my dissertation I did mostly neuroanatomy but some anatomy and physiology, recording _____ in cats and monkeys, and mainly sensorimotor systems.

Farreras: And Coulter was the person you worked with the most while you were there?

Murray: Yes. Coulter was my supervisor, and the other main person there was Bill Willis. Bill Willis is still there. He was then, and he probably is now, the director of this Marine Biomedical Institute, and professor in the anatomy and physiology departments. He's the central figure in that department. He's the main person working on spinal thalamic tract and pain.

Farreras: So how long were you there?

Murray: Let's see, I graduated from Bucknell in '74 and I graduated from UT in '79, so I there for five years. Part of that time I was working as a technician, and then I went to school _____ taking classes.

Farreras: How did NIMH come about?

Murray: Well, while I was in Galveston, I met my significant other, Steve Wise. He had been at Washington University in St. Louis but after we met he actually came to the University of Texas and did a short post-doc, also in Coulter's lab, trying to learn physiology. We were both thinking of leaving Galveston at about the same time and we needed a place to go where we could both get jobs, so we already had the two-body problem. Someone – I don't really remember who – then mentioned NIMH as a possibility. Steve wanted to be _____ recording physiology and I was still really interested in behavior, in neuropsychology. The person I tried to contact was Josephine Semmes. She had a sizable body of work looking at the effects of brain lesions on tactile-discrimination learning, and she had studied patients and monkeys with lesions and I had read some of her papers so I thought this is great. Actually, there's a little wrinkle in that. The truth is, at the time I was trying to contact her, in 1978 or 1979, I found out that she had just left the Lab and had gone into administration, so I don't know when that happened. I think it couldn't have happened too much before I was trying to contact her because otherwise I wouldn't know. And a friend and mentor of mine was calling for me, and the answer was, "Well, Josephine Semmes isn't here anymore, but Mort Mishkin is here, and maybe you should talk to Mort Mishkin." So I did that. I talked with Mort briefly on the telephone. We arranged a time for me to visit NIH, visited, had a chat, discussed what I might do, etc. So I wrote an application for a post-doctoral fellowship, which was pretty standard at the time.

Farreras: Post-docs within the intramural program are funded by extramural funding aren't they?

Murray: Yes, it's the extramural system for NIH fellowships. It just happened that I was applying to use it here intramurally. And the plan for me was to do a project on the somatosensory system using 2-deoxyglucose to do metabolic mapping – something I did not do – because Mort was doing something similar for vision. The idea was to map the system to determine the extent of the entire somatosensory system, especially cortex, in monkey brain. I did start doing that, and after a little while I decided that it was a lot of work that wasn't really going to get me anywhere. That sounds terrible. But at the time, the visual experiments that were going on in the labs, showed clearly that there were parts of the brain where if you stuck an electrode in part of the parietal cortex – I forget if it's PF or PG, part of the parietal cortex – would elicit clear visual responses reported by numerous, numerous people. But using the 2-deoxyglucose technique didn't quite get that brain area to come out as visual. Once I understood that, I said, "Well, what's the point?" So even though this was interesting and exciting, I bailed out and started doing other projects more related to memory, a strength of the [Neuropsychiatry] Lab even at that time. At the time that I came to the Lab, Mort had just developed this delayed _____ test that seemed to be sensitive to lesions in the medial temporal lobe. But, again, all the testing was visual. So I don't remember if I suggested it – I really don't remember how it came about – but it was a natural thing for me to look at tactile memory. I did some tactile-discrimination learning and tactile memory as measured by a tactile version of delayed _____ to sample that I really had to figure out how to implement. It was not so easy. So that's what I ended up doing for most of my post-doctoral years. And at the same time, in those first two years in the lab, what complemented the looking at the neural basis of tactile recognition or tactile DNMS were some anatomical experiments that I was doing in collaboration with David Friedman. David Friedman was a staff fellow in the lab with a somatosensory system background as well, and so we worked together on this project, looking at the connections of somatosensory cortex with the thalamus and somatosensory cortex with other structures that might be anatomically related to medial temporal lobe limbic structures _____ hippocampus.

Farreras: Now this was shortly after the Lab was created?

Murray: I don't know. When was it created?

Farreras: Officially in '77, still headed by Hal Rosvold.

Murray: That sounds correct.

Farreras: Now, I have that by '79 there were only two sections in the Lab: Mort's cerebral metabolism one and Patricia Goldman's developmental and neurobiology one.

Murray: Yes. That sounds right. I was in Mort's section. And, unfortunately, there's no question that Dr. Rosvold had Parkinson's disease at the time, but I think he also had some cognitive deficits because of it. I remember this very clearly.

At first, when I came to the lab, I didn't understand that Rosvold was the Chief. I thought he was this nice old man. He would putter around the lab and occasionally pop his head into my office and ask me a question or something. But it actually was several months before I realized he was the Lab Chief, which was too bad, because I know that Mort thinks very highly of him and has this deep affection for him, I'm sure because he was such a good scientist and mentor. But I didn't know Rosvold in that capacity. Historically, both Mort and Pat worked with Rosvold, so I don't know if he had some projects with each of them at the time or whether he was basically inactive at the time. I don't remember.

And I do vaguely remember when Mort became Lab Chief because everybody in the lab _____ was great and very exciting, everybody except Mort. Mort was not really interested in being Lab Chief, at least that's what he claimed. And I believe it, knowing Mort, because he wasn't interested in doing the administrative stuff.

Farreras: Do you know when Rosvold stepped down? I seem to have information that indicates that it wasn't until pretty late, '86.

Murray: Oh, really? I don't know. I would be surprised if it was that late. I would have put it a few years earlier. But I really don't recall.

Farreras: Because I have those two sections – Mort's and Goldman's – lasting up to 1982, which is just five years after the lab was created. And '82 might help explain the year when Goldman left.

Murray: Yes, that sounds about right. And everyone worked as one big group, with Mort as the head. It was only many, many years later that all of the sections were created anew. So over that entire time, from about 1982 on, there were several people in the lab – Leslie, Bob, me – who ended up staying on in one capacity or another, and other people – Tom Aigner, Tim Pons, Jocelyn Bachevalier – who were all becoming more senior. I mean, if you stay in a place three years as a post-doc, fine. But we all ended up being there roughly 10 years, or for some people, seven. It depends how you came in, seven to 10 years, at which point that staff fellow position was considered to be the equivalent of junior faculty at a university.

Farreras: Like a pre-tenure rank?

Murray: Yes, that's right. So we were pretty independent in a way. People would be earning to do their own surgeries, which, the way the lab worked, is not very common for many people to have had experience working with monkeys when they came to the laboratory. It would be great if you did, but it's pretty rare, so there was a certain expectation that you would have experience working with monkeys before you came to the lab. So to learn how to do monkey neuropsychology and learn how to do aseptic neurosurgery you apprenticed. That's really what it was like. So by the time people had been around three to five years, they might be doing their own surgery and, for the most part, carrying out their own experiments. So I think it was a productive time.

Farreras: So after post-docs worked here for a two or three year period, and assuming they were doing good work, they would then be kept as staff fellows, is that what the next position would be called?

Murray: Yes, it was called a staff fellow, and after two years it was sometimes called a senior staff fellow. At the time those positions went for a maximum of seven years but then something had to happen, it was either up or out. So there was a tenure system, but it has been changing over the years. But my understanding, just from what I'd heard when I first came to the lab, was that in the old days – before I even came – if your Lab Chief wanted you, you were in. You were made permanent somehow. By the time I was coming up for tenure, it was just a standard tenure review process, with a real tenure review committee. They wrote away for letters soliciting opinions _____ about whether you should receive tenure, and it was handled pretty much like it would be handled in a university.

Farreras: Who was on the committee? Lab people alone?

Murray: Oh, no. NIMH people, usually Lab Chief-level people. So fairly senior people.

Farreras: Basic and clinical or, in your case, just basic people?

Murray: Yes, basic and clinical. And then usually one or maybe even two people outside the institutes just for quality control. I thought my chances were maybe 50/50 at the time, for lots of reasons. I think the system has evolved even more now, but if you were a staff fellow in a laboratory at the time when I was, you did not have any independent resources, so you had no budget, no support, except what you got through the Lab Chief. Now it's different. Now there's an actual tenure-track system, and if you are tenure track, you have all those resources. You have your own budget and you usually have a technician or a post-doc or even a couple of post-docs. But at the time you didn't, and yet you were supposed to show some line of research, independent from your Lab Chief. So I don't know how I did that. I knew I needed to do that and there were a few projects that I managed to do independent of Mort, and I feel sort of lucky. I mean, I think it could have gone either way _____. I had a NATO grant with David Gaffen in the relatively early years. They allowed me to do a few projects with David Gaffen and paid for us to travel infrequently, maybe once a year, to Oxford, U.K.

Farreras: Do you know when that tenure system changed? Did that tenure system change or was it before Varmus?

Murray: Well, it's been changing all along, so it was certainly before Varmus. That is, I came up for tenure before Varmus, so I don't know how it changed around the time I got there. I was really clueless. But I think it was with Varmus or under Varmus that the tenure-track system might have been put in place. And I think that's what needs to happen. That's absolutely right. But I remember I was really jealous at the time because here I was, a tenured investigator, and I had maybe a post-doc and a technician, and now you get to come in as a tenure-track person and you're given all this. So I had felt that I had been slogging along without support and without many resources, and all of a sudden there's this big effort to make sure that tenure-track has space and budget, technicians, and I'm going, "Oh, great." But the truth is that that's exactly the way it should be, that's what should be happening, so I think that's good.

Farreras: Yes, a little late for your purposes but nonetheless a good thing!

So at the time you said staff held positions for about two years, but you had a maximum of seven...?

Murray: Yes, senior staff fellow was two years and senior staff fellow was five. For the most part, once people got in those, they would stay in them for seven years.

Farreras: So it was like an inside track position for tenure?

Murray: Well, that's a good question. I think people didn't know whether they would get tenure or not. That is, a lot of people certainly wanted to have tenure at NIH at the end of the seven years, but there are cases where what usually happens is the Lab Chief calls the Director's office and says, "Well, I'd like to bring so-and-so up for tenure," and then they would say, "Well, send us the CV." Then they'd look over your CV and first there'd be some pronouncement about whether you would be allowed to come up for tenure. Sometimes the answer would be, "Well, how about taking a term appointment?" So there are all of these different appointments. A term appointment is another kind of temporary appointment.

Actually, I remember that near the end of my staff fellow position, at the end of the seven years, a couple of people in the lab were coming up for tenure. Tom Aigner was one, I was one, Jocelyn [Bachevalier] was around... She might have been just before me. I remember Tom in particular because Tom and I came up at the same time and we were both offered these term appointments. It would have been something like four more years of some sort of support. And Tom accepted.

Farreras: Was that the standard term, or could they say two years, for example?

Murray: _____. I'm not sure. But I do think you need to keep going beyond three or four years, even though I think it might have been meant as a bridge of some sort. But Tom took the term appointment and I said, "No, I don't want a term appointment." I said, "I'm either good enough to get tenure now or not, and if I'm not, I'd better find out because I need to go somewhere else and get a job." I was interested in the University of Wisconsin at Madison; they had the Harry Harlow Labs. It would have been an interesting place to go to. When I visited it was great. It was just like the Laboratory of Neuropsychology. It was all people in boxy rooms, little test cubicles for testing monkeys. It was funny.

So I said, "No, I don't want a term appointment. I want to come up for tenure. Let's do it," and I really didn't have a lot of confidence, but I felt that was the right thing to do, because I could see at the time that what happened was that a lot of people would hang on in these appointments and go on and on. There was too much of that. And you can end up being four years old and needing to go look for an assistant professor's job. You're better off to just do it earlier if that's what you plan to do.

Farreras: Are you still given the same resources as you had as a staff fellow when you have these term appointments?

Murray: No, things have just slowly over the years _____ a little more here and there. It's an interesting question because a lot of the resources are given out historically, and certainly at the time I got tenure, my understanding of what was happening was that if I got positions, they were in a sense taken away from someone else in the lab, which in this case would have to be Mort's because he's the Lab Chief. So I'm not sure to what extent that's true, but I remember at the time feeling that if I were to have more resources, it would be because they would be taken away from someone else and _____. So I don't know.

I just, gradually, started with one post-doc and one technician.

Farreras: As a fellow, or once you were tenured?

Murray: When I was tenured, after the seven years.

Farreras: Are scientists given a section after they obtain tenure?

Murray: That was about 1989. No, the sections actually came much later.

Farreras: Okay, so even though you're tenured you are still within somebody else's section.

Murray: Yes, that was the way it was set up. But that is different now. I think that if you're tenured now you basically are a section, in the sense that you have your own budget and some assigned space and resources. But that occurred fairly gradually. I was tenured in 1989 but I've actually only been a section for about three years. Leslie [Ungerleider] and Bob [Desimone] were in the Laboratory of Neuropsychology and they became a section first. I'm not sure why, but they're a little more senior. And then I was made a section and Barry [Richmond] was made a section a little later, just a few years later. So I think that this process was evolving over the years, to the point where now the people who aren't even in laboratories, are just freestanding sections.

And I think I stuck with the one post-doc for several years, almost three years, and then I was able to get two people. One person came from Canada with her own money, a grant, and another person came with a McDonald Pew fellowship, and when they were leaving, I said to the Director's office, "Okay, look. I've got these people here on their own money. I want you to replace them. I want you to let me have three post-docs total." And as I understand it, they said okay, so then I had three. But it's not that long ago that that happened, which is okay because there's something about doing this kind of work, monkey neuropsychology, that lends itself to having small groups. It isn't the kind of thing where you can have some megalab, like I think some people do in rat labs, where all the surgery is done by a technician and the technician teaches the post-docs and graduate students. Monkeys are just so valuable, the monkeys themselves. The monkey bodies are valuable, and there's a lot of scrutiny for how things are done, which is appropriate. But because of that and because every surgery is so important, what it means is that if I'm responsible for anything, I'm participating in all of it. Everything my post-docs do, I'm doing. And so a group of three is about right, I think.

Farreras: Do you have any pictures of you or your post-docs doing any surgery or any other type of hands-on work in the Lab?

Murray: You know, to be honest with you, we don't usually take photos of that sort of thing because we are all afraid they'll fall into the wrong hands. So there are lab photos over the years, usually about every two or three years, and there's the occasional photo.

Farreras: I see, yes, I understand. I have some old photographs that Mort [Mishkin] lent us, old black-and-white ones with him and Rosvold, and I think there were some pictures of goats there.

Murray: Goats?

Farreras: Goats! I'll have to ask him about them because I didn't realize that he'd ever done any work with goats. The horns seem to be wrapped up in some type of foil.

Murray: That's hysterical. I've never seen those photos.

Farreras: I think they were Hal Rosvold's and Mort [Mishkin] inherited them when he took over the lab, old black-and-white photos.

Murray: Well, you know, there is the NIH Animal Center in Poolesville, Maryland, which has all sorts of different animals...donkeys, cows, they used to have dogs, and monkeys, but other animals that people would do research on – ungulates and dogs and this and that – it's possible there were some collaborative projects. But I've never heard of this. I'd love to see the pictures, actually.

Farreras: Sure, I'll bring them the next time I see you.

Murray: The other thing that used to be in the lab was chimps. That was before my time, but in the old building – Building 9 – there were chimps that were studied, and there used to be these racks of large cabinets in the hallway that housed the glass slides of brain sections from the chimps. And there were boxes of monkey-brain slides here and there, including in a crawl space in the walls of Building 9. You had to go into one of the test rooms, and there was a little doorway and a crawl space literally about two or three feet wide. I don't understand why these things were built that way, but it was built like a double wall, and the boxes of brains had been stored in this crawl space. Maybe they're still there. I don't know. When we moved out, I think we tried to clear out.

Farreras: When was the move from B9 to B49?

Murray: I don't know. I don't remember the year.

Farreras: But when you came, you were in B9?

Murray: Oh, yes. I was in Building 9 many years, and we lived through two or three renovations because all the regulations regarding surgery especially and the facilities for animals were changing over the years, and the facilities were pretty old to begin with. So to meet the regulations, we would have to keep living through these renovations. So I don't know how long we've been here. Six years at least. And this building was built to be a more state-of-the-art building for working _____ primates, so not everyone, but many of the NIH people working with monkeys were in this building. It's nice, better facilities for sure.

Although that was a shock, too. I think Building 9 – I don't know if you've been in it, it's one of the old, small, red brick buildings on that side of campus – was meant to be a temporary building, and at one point we had almost the whole building to ourselves. In other words, there were a few pieces of other labs there, but almost all the building, Building 9, was the Laboratory of Neuropsychology. So it was great. We just walked down the hall. They had a big blackboard in the hallway with a little calendar of the scheduled surgeries – because that's the way we could keep things straight – and if you wanted to schedule a monkey surgery, you just went down the hall and you could see what else was scheduled, and since you could only have one or maybe two a day, you'd pick your date accordingly. And likewise, you could just walk down the hall and find the fellow who ran the surgeries and say, "Oh, I want to do a surgery tomorrow." "Okay," and that was it. Now, in Building 49, because it's not only the Neuropsychology Lab but there are several institutes in the building, there is much more of a corporate feel. We submit a form, like the Surgery Request Form, and you have to do this about a week in advance. So the facilities are great, and the surgery facilities are wonderful, but it is a totally different phenomenon. I mean, you almost don't know the people who are running the surgery. So that took some getting used to.

Farreras: I noticed your change of title, which Mort [Mishkin] also changed – to research physiologist – in '89. And he said that that was switched because of the higher status and higher pay of such a title, that the psychologist was lower on the scale than an M.D. and that the title of research physiologist allowed for a higher salary and higher status. Is that accurate?

Murray: It could be, but not that I know of. My understanding was that in 1989, I would have been a research physiologist because I became tenured. I entered the GS system. But before that, I wasn't. You're not in the GS system.

Farreras: Oh, before you're tenured, you're not in the GS system?

Murray: I think that's right, yes. There may be some people who are in there some way, but for the most part, my recollection is that if you're a Ph.D. you start at GS-13, and so, as part of the GS system, you are given one of these titles. And the reason I'm a research physiologist is because that's what my degree is in. It goes with your academic background. I know there are people in NIMH, for example, who, when they were tenured and in the GS system, were research psychologists because their academic degree is in psychology. Some of them are research biologists, have their degrees in biology, and mine was in physiology, so I'm a research physiologist. I think that's all it meant. And maybe there is something about salary, but I wasn't aware of it if there was. Actually, the salaries go with the GS level, and it's true, you could be the same age, have the same experience, doing the same experiment, as the M.D.s and as I understand it, the M.D.s would be paid more. I don't know what happens to them in the hiring and personnel _____.

Farreras: I thought it was to make up for the money that they would not be making in private practice.

Murray: I think that's right.

Farreras: Was there any contact between the members of the new Neuropsychology Lab and those of the Psychology and Psychopathology Lab that it emerged from? Leslie [Ungerleider] said she didn't have any contact at all with them, even before the Neuropsychology Lab was established.

Murray: Very little, unless they came to visit. And I'm not even sure I know who was in the former Lab. I think I did talk to Josephine Semmes once or twice on the telephone, mainly to ask her a question about something, but I never met her in person. So nothing protracted and serious.

Farreras: Were there any collaborations with other labs within NIMH or with other institutes? Or were there more collaborations within the lab?

Murray: Interesting. Oh, there have been some collaborations over the years with other labs, maybe other institutes. Again, for many years it was really Mort's [Mishkin] lab, and so it's not like you have a lot of resources to start doing extensive collaborations. But, for example, during the '80s, I was doing a collaboration with Steve Simmey, who was in the Comparative _____ Lab, in the Child Health Institute, and that worked out well.

Farreras: Are these mostly scientist-initiated collaborations?

Murray: As opposed to what? I think the answer is yes, but...

Farreras: I wondered whether Congress or the administration would say, "we have a lot of interest in topic X," like the current interest in bioterrorism, and whether that would initiate or influence collaborations...

Murray: Yes, sometimes they come out that way, so I'm not sure I can tell you about all the collaborations that might have been going on. I know Mort [Mishkin] had some collaborations with Simmey and I had a separate collaboration with Simmey.

I also had a fairly lengthy collaboration with people in the Laboratory of Cellular Biology and NIMH, Lee Iden and Diane Roush, because there was money targeted for AIDS research, and Lee Iden was interested in immunology and viruses, so he initiated and spearheaded a project examining the effects of SIV in monkeys – simian immunodeficiency virus – which was related to HIV in some way. It was really a big project. And my little part of the project was to try to understand the neuropsychological status of SIV-infected monkeys. And we did that and got a nice little *Science* paper out of it. That was the first paper. And it wasn't even conducted here at the NIH. It was carried out at a contract facility in Rockville. It was a worthwhile project. It was a bit hard because it never felt like we were really being supported for doing this project or that anyone particularly cared about this project, although Lee was really serious about it and he really got good people to collaborate in all of the different aspects of it that we needed: histologists _____ neuropathologists and _____ behavior, and his expertise was already more _____ molecular biology. So several papers came out of the project but after a while, it sort of petered out because of lack of funding, I think. So that went on for several years. But that was definitely in the category of something that might not have been initiated if it weren't for some targeted funds. And there were several years where there was targeted money and there were often calls to do AIDS-related research.

Farreras: How do you, as a scientist, hear about that? How do you find out?

Murray: Sometimes it trickles down through channels like the Lab Chief; nowadays it would be e-mail _____ initiatives on things. There were years when there have been some targeted monies for schizophrenia research _____ AIDS research, things like that, but...

Farreras: But you're still ultimately left to decide whether you want to participate in it?

Murray: Yes, that's right.

Farreras: You're not assigned a topic, "Here, work on this"?

Murray: That's correct, although I suppose that could have happened at some time in some place. It never happened to me. Sometimes I'm not sure it was the best decision for me to have gone to work on this SIV-related project, but at the time I thought it was a real public service and I really should do this, and it seemed like it wouldn't hurt what I was doing here. In other words, it's not like I would be using my resources and _____ to be doing that project. Those were physically separate and budgetarily separate resources, so I thought that that would be good overall. So I went ahead.

But I also think I was very naïve. Certainly when I started I was very naïve because I thought, neuropsychology, SIV-infected monkeys, no problem. If they have problems, it's just because they have frank brain damage, and we'll see brain lesions and it'll all make sense in terms of what their impairments are. And, of course, as it turned out, it was much more complicated than that. They don't have obvious, frank brain lesions, even using the strains of SIV that are known to have some sort of neurological effects. It was much more complicated than that and probably much more like HIV, which is, some animals showed fairly early cognitive deficits and there seemed to be absence of any illness, and others didn't, and different animals had different problems. Of course, what we were trying to do was correlate any cognitive deficits with other cellular, molecular changes, but that was harder to do. So there are a few changes in somatostatin and blah-blah-blah, but it wasn't anything obvious or clear that we could say was the cause of cognitive impairments.

Farreras: I see. Well, we talked about Mishkin taking over the Lab, and I'll double-check on that '86 date since it seems pretty late. How did he become Lab Chief? Was he appointed by an NIMH Scientific Director at the time or...?

Murray: I don't know. I think that must have happened. In some sense he was the clear person to head the Lab. He was the senior person who was there at the time and he was obviously very capable and _____.

Farreras: So they would never think of bringing someone from the outside to head it?

Murray: They could have, but I don't recall any discussion along those lines, and he was just a natural for that position. But I don't know what happened. I know in other situations now, they often do think about bringing in someone from the outside. But I don't know what happened at the time. Again, from my vantage point, being a little lower on the totem pole, I just remember that it happened.

Farreras: Did anything in the Lab change once he took over? Were there any major changes that resulted from the change in leadership?

Murray: I don't remember any. The truth is he was already heading his own group. If anything, I think things may have slowly grown over the years because Pat Goldman left and there was space and positions to be filled. I think our group did grow over the years.

Farreras: Did Rosvold retire? What happened?

Murray: Yes, Rosvold retired.

Farreras: And then, was he here at all, or was he here but didn't really lead the Lab at the time?

Murray: When I first came, he was definitely in the lab and he was not yet retired. And I just frankly don't remember whether he was conducting scientific projects or not. I don't recall. One thing I do remember is, when Pat Goldman left, there was the infant monkey nursery, and some decision had to be made about what to do with the infant monkeys. And actually, that's where Jocelyn Bachevalier came in, in a sense. She and Mort [Mishkin] decided to look at the development of memory and neuropsychology. I don't know when that started, but I guess it was about the time Mort [Mishkin] became Lab Chief, maybe even before.

Farreras: Why did it take so long for the new sections to be created in '92?

Murray: I don't know. I think no one thought there was a need; there certainly was this old, very hierarchical, European kind of system where the Lab Chief is in charge, is responsible for everything that goes on in the Lab and for apportioning resources, which is fine, but it's not clear in that system how new people and new scientific areas are created. That's just the way it worked. But it worked pretty well, actually, and I think Mort [Mishkin] is widely credited with bringing together good people to work on his vision of what he wanted to do, looking at the neuropsychology of learning and memory. And he had Leslie [Ungerleider], who was for many years like the anatomist, and Bob Desimone, the neurophysiologist. Barry Richmond was also a neurophysiologist. And I was doing mainly neuropsychology. So we were all working on these really closely related questions. I think that is unlike many other labs. And I think it was very powerful in many ways. You could just walk down the hall and say, "What did so-and-so say about this?" and "Do you have this paper on the hippocampus?" It's just amazing, and it was that informal. We were all pretty junior and it was all very open, and you'd just go say, "Hey, tell me about this," and "What about that?" and that's something that I think we can't do now. We're all too senior, we're all too busy, we all travel too much, and it's not quite like that anymore, but that's what it was like then.

Farreras: How were people brought in? You were already here on a post-doc and I think Leslie [Ungerleider] was also here on a post-doc, and Bob Desimone...

Murray: Came from Princeton. I believe Bob came as a staff scientist, but I think he was doing a post-doc under Charlie Gross at Princeton.

Farreras: So, what criteria would be used to bring people in?

Murray: Well, I imagine that it's actually pretty similar to what is brought in now; that is, if you're trying to run a research program one thing you try to do is bring in people to fill in different niches.

Farreras: So there's an identified need first, and then you bring people in to fill it?

Murray: I think that's true. I can't tell you what Mort [Mishkin] was thinking at the time, but I think clearly Josephine Semmes had left the lab, and I was this person interested in the somatosensory system, which is sort of a niche _____, and I don't think there was any idea about how long I might stay around. I think it wasn't a problem with _____ post-doc. You just take it and see what happens. So I don't know, after that, what Mort [Mishkin] had in mind. But it's not always easy just to say, "Oh, I want someone in this area," or "I want the best person." You have to work with what you've got. But I think because Mort [Mishkin] is such a strong personality, I think he kept people interested in the questions he was interested in and got them to do _____ in investigating those rather than launching off into some more arcane area. It's good to have people go off and have their own interests, but at the same time you want to have some central theme. So I don't know what his particular criteria were. I think it's always good to _____ come to you when they're interested and they're interested enough to bring their own money or to contact you and say, "We're interested in working with you at the start."

Farreras: I see. So I have 1992 as the date for when the first section started, Cognitive Neuroscience under Mort [Mishkin], Neurocircuitry under Leslie [Ungerleider], and Behavioral Neurophysiology under Bob Desimone.

Murray: That sounds about right.

Farreras: And then two more in '95.

Murray: Yes, that sounds right, for me and Barry.

Farreras: Actually, I have Al Mirsky and James Haxby.

Murray: That would have been in Leslie's Lab.

Farreras: For a while, both sections were listed under both the Lab of Psychology and Psychopathology and the Neuropsychology Lab, and then the two in '96, Neurobiology of Learning and Memory, which was yours, and Barry Richmond's, Neural Coding and Computation.

Murray: Yes.

Farreras: And then those sections were then moved into the new Brain and Cognition lab. But for a while they seemed to be listed as having joint appointments or something within each Lab.

Murray: Yeah. I don't know why that happened, and to be honest, I wasn't even aware of it. I know that they became part of the Brain and Cognition Lab. I suspect there was just some need to cover something administratively, and that's what they did.

Farreras: Until she became Lab Chief?

Murray: Yes, because Mirsky or Haxby were never here; they never came to the Lab and then left again. I think that was just on paper.

Farreras: Okay, so Mort [Mishkin] stayed here, Bob Desimone stayed here, and yours and Barry Richmond's stayed here.

Murray: Yes.

Farreras: There were four sections in the Neuropsychology Lab then?

Murray: Yes, that's right. But we all kept doing what we had been doing all along. So, again, it was more some administrative action.

Farreras: So it wasn't that any of the three actually wanted a section of your own?

Murray: Well, I think there was a movement from the top, from the Director's Office, to have tenured scientists be truly independent in this way, to be independent sections. Otherwise, how do you keep track of them? How do you deal with budget cuts? How do you do anything? For instance, I don't know how they would fit the tenure-track person in the tenure-track system that they use now, but they also didn't do this for tenured investigators. So it does make sense to have it organized that way. Once you start thinking about it and once you start going down that road, then every tenured investigator ought to have his or her own budget. I suppose there's a little more flexibility with that system. There's no question there's more flexibility for the director to, literally, direct resources, so if a particular investigator says, "I need X, Y, and Z," then they can address that specifically, whereas before, if the resources were through the Lab Chief and there were no Sections, it's hard to do that. You really have to let the Lab Chief direct the Lab and _____ resources. So there's a lot more control from the Director over what happens. So not at our most recent Lab review, but at the one before that, three years ago, I asked for more technical support and I was granted that, so now I have on paper two technicians and three post-docs. And I think I could probably have an additional post-doc. And there's another source of support now, which are called the pre-IRTAs. These are the post-baccalaureate intramural research training awards that are for students who have a B.S. or a B.A. and they think they want to go to graduate school. That's the concept _____ - and they want to work in a Lab for a few years. So it's sort of like a technician. Actually now I have four of these pre-IRTAs working in my group, which is great. So that's actually a lot more support of the kind that we didn't have before.

Farreras: Were the technicians you would get earlier be already trained technicians?

Murray: Not necessarily. If you hire a technician, you just get what you get. Often, if they're just graduated and just have a B.A. or B.S., they're not going to know what you want them to do, so you train them. But it's like any other venue. There may be people whom you steal from some other location or that come from some other place that already have experience. That can happen. That is possible.

Farreras: I don't have very much else for today. I asked Leslie [Ungerleider] about the move from Neuropsychology to the Brain and Cognition Lab, but I don't know how much the people here were involved with that or whether that was something that the administration had to do with the earlier Lab and the change in name...

Murray: I don't know. Functional Imaging was just coming into its own, and Leslie wanted to pursue functional imaging – she was doing it already for several years – and that became available and it was sort of a natural thing _____ like that.

Farreras: Do you know where that push for neuroimaging came from? I asked Bob Cohen, who was the Clinical Investigations Director until 1981, why not just do away with the Psychology and Psychopathology Lab altogether, since there were few of the sections and investigators left, and just create a new Lab of Neuroimaging instead?

Murray: I thought they did that; I thought they did essentially what you just said, terminate the original Lab.

Farreras: I assumed that because they ended up taking in the people who were in the previous Psychology and Psychopathology Lab, like Mirsky and Ted Zahn, that the Brain and Cognition Lab was more of a continuation of that original Lab.

Murray: I wouldn't put it that way. No. My concept of it was that it really is a new Lab with a new mission, and there was a need for a Lab like that. I think it's fair to say that someone thought there was a need and it's probably a good thing that we have this Lab. We can argue about how it could have come about. For example, I suppose it could have been a section in the Laboratory of Neuropsychology, but that's not what it is. It's a Lab, and there are Sections within that Lab, as you know. At the time, my understanding was that they really wanted Pls like Al Mirsky to be in a Laboratory, but I think that's not true now. But now there are these free-floating Sections that we already talked about. But at the time they really tried to keep people in a Lab framework and in a Lab setting, which I think makes it easier, administratively, when they're reviewed, to be reviewed as part of a Lab. It has a place to be in their *Annual Report*. It's a place to file it. So I'm not sure that's true. And I guess it is natural, in a way, that Al Mirsky would go into Leslie [Ungerleider]'s Lab. At least she's a psychologist. And it's true, I guess, that his work didn't really fit into the mainstream of functional imaging, but it was not a bad match in some ways.

Farreras: Well, what about scientific careers at NIMH versus academia or industry?

Murray: Well, I know pharmaceutical companies _____ want to do monkey behavioral pharmacology _____. What can I say? It's great. I think the truth is, for monkey neuropsychology right now, it's so expensive that it's just almost impossible to do it anywhere else. There are only a handful of places where people even do monkey neuropsychology, at least historically, Oxford University in the U.K. has been one; University of California at San Diego has been one; McGill University in Montreal; _____, and Northeastern University and Boston University, and some of the primate centers. So not as many as you might think. It seems like such a natural thing that you would have monkey neuropsychology going on at the primate centers, but there's not. There's some at Davis Center and there's some at Madison, but, again, not so much affiliated with the primate centers as you might think.

Farreras: So the access to such research and such resources...

Murray: Yes, I feel really lucky to be here. I think it's just a function of how difficult the work is but also how expensive it is, at least my sense of it is, so that no one has come up to me and said, "Oh, Betsy, come to our university, we want to build you a primate lab." That's just not in the cards. So I feel very lucky to be able to be here and do the kind of research I do. Well, because I've been here my whole career, I can't really say what it would be like to be at a university. I think it would be nice if there were more students around. The NIH isn't as scholarly or doesn't have some of the resources and structure of universities, but that's _____.

Farreras: In terms of mentoring or teaching, or...?

Murray: Well, for example, having graduate students around. The few graduate students whom I've had contact with, because they've been in my Lab but getting a degree at, say, George Washington University, would bring in different questions coming from different directions and they force you to think about things in a different way than you would without them. Even post-docs are too developed for that. You don't have these off-the-wall questions that you get from undergraduates and graduate students.

Farreras: So they have an academic affiliation and they're here on internship or on summer work _____?

Murray: It depends. Sometimes they might come as a summer student, which we can still do, but now, occasionally a student sometimes comes from local universities but also from far away. Just as long as everybody's on the same page, as long as their supervisor is happy for them to be here in the lab and we're happy because they're doing good things, then they can come work here in the lab and do a monkey project in partial fulfillment of their Ph.D. I think that some universities aren't happy with that arrangement because they want the students teaching and doing things at the university. But it's worked out great with the students I've had. And now with these relatively new pre-IRTAs we are now able to support them in the lab even while they're working towards the Ph.D. So I think that's really wonderful. It certainly makes it easier. I think there have always been some graduate students but I think it was more difficult because there was no method for paying and supporting them, which there is now.

Farreras: And how about as a female doing monkey work? I know Leslie mentioned a study that Sue Swedo did a couple of years ago on the female scientists at NIH...

Murray: Yes, there is an interesting Sue Swedo story. Sue was what's called NIMH Women's Science Advisor at the time. I actually did a stint in that sort of position, an honorific position of sorts. It's meant to help women maneuver the NIH with whatever they might need: mentoring, information, you name it.

So, Sue Swedo did this very straightforward analysis of years from Ph.D. versus salary for different categories of employees and the men's regression line was up here and the women's regression line was down here. So she took it to the Scientific Directors and there were some criticisms about how the analysis was done, and she said, "Okay, fine," and she would go redo it, come back, and it would get worse every time in terms of the divergence of salaries. And to her credit, Sue kept at it. My understanding is she finally took it to a lawyer and just said, "Well, look. These are the data _____ and I think looks like. Is there a case here?" and they said, "Oh, yes." And so, very shortly thereafter, the NIH came to some agreement with women. And my understanding is that there were about six women, of whom I believe Leslie and I were two, who were just way below the regression line, and there was a gap even between a lot of the women, and the six other women. So in the end what they did was take these six lowest-paid women and just moved them up to the men's regression line in a pretty straightforward manner. And we signed a Memorandum of Intent essentially saying, "Yes, we agree to this big salary raise, and if you give us the salary raise, we will promise not to sue you." And we had this nice salary raise. It was amazing. For me, the salary raise that I was supposed to get couldn't be accommodated in my GS level, so they raised it as far as they could. It was an academic promotion, so I had to wait to be promoted through the usual promotion, and then be jumped up again to get the final thing.

So, yes, I'm forever indebted to Sue Swedo. But that's part of what the NIMH Women's Science Advisor and other Institutes _____; I think since that time, most Institutes are at least keeping track of the numbers and trying to make sure that doesn't happen again.

Farreras: I will try to set up a meeting with Sue Swedo as well and hear her recollections of the early NIMH years as well as this recent analysis you and Leslie talked about.

Well, I want to thank you for meeting with me and devoting so much time to this oral history.